

## Again on the nomenclature of frogs

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The author answers the various criticisms recently expressed by SAVAGE (1986) on his own works, and shows that these are due to severe misunderstandings of several important rules of nomenclature. A particular attention is given to the problems raised by the distinction of different types of names and spellings (new name, new replacement name, unjustified emendation, incorrect subsequent spelling), and several examples are treated in detail. Some of the suggestions of SAVAGE (1986) concerning family-group names are shown to amount to a proposal of a return to pre-Code rules for these names, since this author does not accept the principle of priority for them. The question of the rules suggested by DUBOIS (1984) for the nomenclature of class-group taxa is also discussed, and the modifications recently proposed in this respect by LESQUIRE, RENOUS & GASC (1986) are criticized.

*Note* — This paper was judged too long by the editors of *Copeia* to whom it had first been sent. In order to save space, the various papers by DUBOIS cited below will be referred to using code numbers, which are made explicit in the bibliography. The abbreviation ASW refers to FROST's (1985) checklist.

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## INTRODUCTION

The journal *Copeia* recently published a review by SAVAGE (1986) of a paper of mine on the suprageneric nomenclature of anuran amphibians (D-26). It so happens that this author, who is a member of the ICZN, disagrees with me on several important nomenclatural points, but that I am not at all convinced by his arguments. I am answering these comments in detail below. As will be shown, some of the points in question are not unrelated to some of the comments I made elsewhere (D-41) concerning ASW.

SAVAGE's (1986) comments bear on different aspects of my work, which will be discussed successively below : suggested rules for the nomenclature of class-group taxa ; distinction between different types of generic names or spellings ; valid family-group names ; classification used. As will be shown, SAVAGE's (1986) disagreement with my work comes from two major sources : disagreements on the interpretation of the *International Code of zoological Nomenclature* (ANONYMOUS, 1985 a) ; and misunderstanding of some parts of my (French) text.

An initial comment. SAVAGE (1986 : 259) writes : "Unfortunately Dubois would have been better served had he waited for the appearance of the revision (3rd ed.) of the International Code of Zoological Nomenclature (1985) to appear (sic) instead of basing many of his conclusions on the 2nd ed. (published in 1964 and amended in 1974)." This is a rather strange statement indeed. The "short work" (as SAVAGE, 1986 : 259, calls it) in question was the result of a 4 years study. It was finished and sent to printer in 1983, and published in 1984. On the other hand, the 3rd. edition of the *Code* had been announced for several years already, and there was no assurance that it would appear in 1985 rather than in 1986 or even later. SAVAGE (1986) is not serious when he says that everything should have stopped in the world, or at least that no nomenclatural work should have been published "just before" the publication of this 3rd. edition. The question in fact must be reversed. This 3rd. edition introduces significant changes in some articles of the *Code*, and indeed I immediately corrected my own work just after the new *Code* had appeared, in a paper (D-29) that SAVAGE (1986) ignored, although it was published in August 1985. But, as was pointed out in this paper, the changes introduced in these articles of the *Code* are largely open to criticism, since they generate an unavoidable new cause of confusion and instability (see also below). It would have been the responsibility of the authors of this new edition to avoid introducing such new causes of disruption of nomenclatural stability, and not of systematists to stop working until the new edition appears. Now, this new edition, which was clearly prepared too quickly, is undoubtedly open to strong criticism (see e.g. DUPUIS, 1984 ; DUBOIS, D-29), and will certainly have to be modified in some of its parts in the future. To follow SAVAGE's suggestion, should all systematists stop to work on nomenclatural matters, in the expectation of the forthcoming 4th. edition of the *Code*? Let us hope they will not, although it is clear that a "final" *Code* will never exist.

## SUGGESTED RULES FOR THE NOMENCLATURE OF CLASS-GROUP TAXA

SAVAGE (1986) provided a shortened translation of some of the rules I proposed (D-26) for the nomenclature of class-group taxa (i.e. all taxa above the family-group). This trans-

lation is correct, although abridged, for most rules. Two errors of translation must however be pointed out. In rule No. 4, the last part of SAVAGE's sentence ("and were not created for taxa superior in rank to those of the class-group") is completely invented : since I proposed to consider *all* names above the family-group as belonging to the class-group, there exist no taxa superior in rank to those of the class-group ; even the names of highest ranks, such as those of phyla and kingdoms, do belong, in my opinion, to the class-group, for the reasons explained below. This last part of the sentence should be replaced by the following : "even when they were not the first ones to have been created for the higher taxa in question." In rule No. 6, the first part of SAVAGE's sentence ("In the absence of an international consensus among several old names that have had considerable usage it is preferable to choose..", italics mine) is incorrectly translated and should be replaced by : "In the absence of any international consensus and of old names having been used for a long period, it may be preferable to choose..". As will be acknowledged by those who will look carefully at these sentences, the errors of translation sensibly modified my proposals, making them more open to criticism. Besides these errors of translation, it should be added that my proposals were not limited to these six rules. I also proposed to generalize the use of the type concept to the names of the class-group, and suggested that the types of class-group taxa should be *genera*, not families or other taxa. These type genera would be designated either by (original or subsequent) monotypy, or by subsequent designation among the originally (or subsequently) included genera of the taxon.

Recently, LESCURE, RENOUS & GASC (1986) suggested to modify my proposals, in two respects : (1) they proposed to recognize not only one class-group above the family-group, but a class-group and an order-group; (2) they suggested that types of class-group taxa should be families, and types of order-group taxa should be classes. I disagree with these proposals, for the following reasons.

To separate higher taxa (above the family-group) in two different groups would be extremely artificial, because many names first created e.g. for an order were later applied to a class, or the reverse. If two distinct sets of names were to be recognized, that means that the principles of priority and of homonymy would apply independently in these two groups. Thus, when e.g. a name created for a taxon considered a super-order by its author is transferred to the category subclass by a subsequent author, it would then have to be treated as a new name, with its own author and date. This would complicate very much and most unnecessarily the matters. Furthermore and above all, if class-group and order-group names were separated, the principle of homonymy could not apply between them : this would allow the possibility that the same name be beared e.g. by a class and by an order (either included in the class, or in another class, possibly in a quite different group). This would be a strong source of confusion, and should be avoided. The principle of homonymy is an important principle of the *Code*, which should not be underestimated. Due to the very high number of species-group names, it cannot apply in an absolute way at this level, where it only applies within a given genus. But at higher levels, it applies within the whole animal kingdom : no two animal genera or families can have the same name, no matter how far these taxa may be in the phylogeny and classification. I think the same should apply to all animal taxa above the family-group. This is all the more justified that the number of taxa at this level of classification is extremely low as compared to the numbers of taxa at lower levels and there would be no point in distinguishing two independent sets among this low number of names. Fi-

nally, if a distinct group of names was to be recognized for orders and related taxa, the same should be true for phyla, kingdoms and other higher taxa.

In reality, there exists at present no theoretical reason for stating that a higher taxon is, say, a super-order or a subclass. These categories are only successive steps in a hierarchical scale, and if new dichotomies are added, new taxa recognized, or, on the reverse, if taxa are merged, the place of many taxa in the scale may well move up or down. It is true that the same also occurs for taxa of the family-group and of the genus-group, but nevertheless, at least at the genus level, some rules may be proposed to reduce the part of arbitrariness in the allocation of taxa to given categories (see DUBOIS, D-12, D-14, D-30). There seems therefore to exist neither practical nor theoretical justification for recognizing two distinct groups of names above the family-group. These principles had guided me in my proposal of a single class-group for all these names, and I had to explain them here in full detail only because this proposal was challenged.

It was also after reflection, and not by chance, that I suggested that nominal class-group taxa should have types, and that these types should be nominal genera, not families, or orders, etc. As a matter of fact, a hierarchical succession of types would only unnecessarily complicate the nomenclature of higher taxa and would create additional difficulties at any change of diagnosis of the higher taxa and at any transfer of included lower taxa from one group to another. In the family-group, all taxa names are based on generic names, whatever their rank : a familial name is not based on a subfamilial name, a subfamilial name on a tribal name, etc. Thus all taxa of the class-group should, for more simplicity and clarity, be based on names of the same category. Then, why not choose the family, rather than the genus, for this category? For two reasons: (1) since family-group names cannot be, like class-group names, entirely new names, but are based on generic names (and formed in a very strict manner on the basis of the stem of these names), to designate a type family is strictly equivalent to designating a type genus (on the name of which the family name is based) and brings no additional information ; (2) many old class-group names were created at the beginning of systematics, when the category family still did not exist or was not in common use (it began so only quite after the beginning of the XIX century). Therefore, these taxa were created without included families, while they were in most cases credited with included genera at their foundation, the category genus being in use since LINNAEUS. Now, the proposal of LESCURE, RENOUS & GASC (1986) would make more difficult, in many cases, the determination of the type taxon of a given class-group taxon. In all the cases where the taxon was created with no included family, the "originally included families" would be the first ones to have been referred to this taxon by subsequent authors, and this information may be difficult to trace. Another possibility would be to consider that the originally included families are those based on the generic names cited as included in the new taxon at its creation, but : (1) this would be strictly equivalent to recognizing type genera (rather than type families) for these taxa ; (2) the originally included genus (or genera) may not be the type genus (genera) of (an) existing family-group name(s), which would cause an additional problem. For all these reasons, I think it justified to maintain my initial proposals and to refuse the changes proposed by LESCURE, RENOUS & GASC (1986).

To come back now to SAVAGE's (1986) comments, he seems to accept my proposals, but he adds rightfully that to have universal value these rules should be adopted by the ICZN after discussion : this is true, but before they could be discussed these rules had to be pro-

posed by someone. SAVAGE (1986) does not seem to reject my rule No. 3 ("A class-group name that is a junior homonym of another class-group name must be rejected") Then, why does he state that the names *Archaeobatrachia* Reig, 1958 and *Neobatrachia* Reig, 1958 are "perfectly good names", when I had shown (D-19, D-26) that these names are junior homonyms (despite the one-letter difference) of SARASIN & SARASIN's (1890) *Archaeobatrachi* and *Neobatrachi*? Also, he states that my rule No. 6 creates difficulties, and he seems to suggest that a strict priority rule should be followed. However this proposal is contradictory to my rule No. 4. To take only the example of anurans, if a rule of priority was to be followed, neither the names of NOBLE (1931) nor those of STARRETT (1973) would be the valid ones for the suborders of this order. Numerous much older names do exist (see KUHN's 1967 list, which furthermore is incomplete), and strict application of the rule of priority to this case would require the resurrection of names which have never or almost never been used and which are almost completely forgotten now. This is the reason why I had expressly stated (D-26 : 9) that, at the class-group level, the principle of priority should not be strictly applied.

Finally, it is the full right of SAVAGE (1986 : 261) to "like subordinal names such as *Archaeobatrachia* or *Lemmanura* better than *Discoglossoidei*", but let us note that class-group names such as *Discoglossoidei* have not only been proposed by SOKOL (1977) in Anura, but also by LESCURE, RENOUS & GASC (1986) in *Gymnophiona*, and are currently universally accepted as valid by all authors in Urodela (see e.g. ASW), so that, at least for amphibians, names of this kind have tended to gain a growing acceptance among systematists.

## DISTINCTION BETWEEN DIFFERENT TYPES OF GENERIC NAMES OR SPELLINGS

### INTRODUCTION

As unpleasant as it may be to some modern taxonomists, systematics is not a young discipline, and it has undergone many changes since its beginnings. These changes have been particularly drastic in that part of systematics which deals with the names of taxa, i.e. nomenclature. The beginning of "modern" nomenclature is arbitrarily fixed in 1758, at the publication of LINNAEUS' 10th. edition of the *Systema Naturae*, but it is clear that at this time almost no rules existed. The need for nomenclatural rules appeared and developed only with the increase of the number of known taxa and of their zoological names, and led to the creation of international Rules of zoological Nomenclature, which have been the matter of various modifications and of several editions, the last of which appeared very recently (ANONYMOUS, 1985 a ; see pp. xv-xvi of this book for a very brief history of the current *Code*). It is clear that works published prior to the establishment of any set of rules could not follow them. For example, no principle or priority was followed by many authors in the XVIII and XIX centuries, and even quite late in the XX century some authors did not follow it in all cases : e.g. in amphibians BOULENGER as late as in 1920 still rejected some names (e.g. *Polydectes afghana*) for "inappropriateness", although he clearly acknowledged that they were senior synonyms of names considered by him as valid.

The same applies to all the other rules of the current *Code* (principle of homonymy, rules for the designation of types, rules for the formation and treatment of names, rules for

the authorship of names, etc.). This is important to bear in mind when in 1986 we use the 1985 edition of the *Code* to analyse old, pre-*Code* texts. I have already stressed on several occasions (DUBOIS, D-21, D-42 ; BOUR & DUBOIS, 1984) the fact that the application of the *Code* to such texts "must be made with care, understanding and intelligence" (BOUR & DUBOIS, 1984 : 357). For example, the concept (and term) of lectotype was created only at the beginning of our century (SCHUCHERT & BUCKMAN, 1905) and became widely used only in the second half of this period ; it is therefore irrelevant to reject a lectotype designation which was made in the older times by the simple use of the term "type" (see e.g. BISCHOFF, 1982 and DUBOIS, D-42).

Similarly, to decide if a new name was proposed as the name of a new taxon, or as a new replacement name (*nomen novum*) for a taxon already recognized by previous authors requires a detailed and careful analysis when old texts are involved. Here we come to the main criticism of SAVAGE (1986) concerning my work. SAVAGE (1986 : 260) writes : "[Dubois] apparently believes that the citation of a previously proposed name in the synonymy or footnotes associated with the proposal of another name by a subsequent author makes the latter name a new replacement name." This statement is totally unfounded, since I never wrote or even believed such things. My judgments concerning the status of old names never rely on "general rules" like the strange rule invented by SAVAGE, but on a careful analysis of the texts themselves, as is shown in my previous works on similar problems (D-8, D-11, D-17, D-18, D-21, D-24, D-26, D-29, D-40). In this respect, I disagree with SAVAGE (1986) and also with HOLTHUIS (1983) on the status of the generic name *Dendrobates* and of several other generic names created by WAGLER (1830 b). To save space in the *Bull. zool. Nom.*, and because I thought all interested biologists could go by themselves back to the original texts, study them honestly, and come to the same conclusion as myself, I wrote : "Instead of discussing this in detail in this *Bulletin*, I think it simpler to refer the readers to WAGLER'S (1830) original text itself. Other arguments could also be found by studying the other publications of WAGLER." (D-21 : 198). However, SAVAGE'S (1986) paper shows that I had been too optimistic. Since despite my previous papers on this question (D-18, D-21), some people remain unconvinced by my arguments, it is necessary to come back to the question in more detail, as heavy and space consuming as it may seem.

Before going into the details of WAGLER'S works, however, it will be useful to discuss these matters at a more general level.

#### DIFFERENT TYPES OF GENERIC NAMES AND SPELLINGS

When a systematist uses a generic name in a scientific publication, he may use an existing name, without changing it or with some modifications, or a new name. For the sake of clarity, the different possible cases may be presented in the form of a dichotomic key.

I. The writer uses a new generic name for a genus which he considers new (even if including already named species): both the *taxon* and the *name* are new, and must be credited to the author of the publication. In modern times, the new name is generally presented with the indication "gen. nov." or "n. gen." In older texts, indications like "mihi" or "nobis" were sometimes used, but in many other cases the fact that the name and taxon are created as new in the paper must be inferred from other direct or indirect sources of evidence.

II. The writer deals with a genus which he credits to a previous author (even if he modifies in part the diagnosis or the contents of this genus, e.g. in retiring from it or adding into it some species). This reference to an already existing taxon is generally explicit, but in some cases must be inferred from the context : for example, in older papers many batrachologists used the names *Rana*, *Bufo* or *Hyla* without ever mentioning their authors, but it was however clear that they were not creating new names or taxa. In this case the *taxon* is not new (although it may be somewhat emended). As for the *name*, two possibilities appear.

A. The writer uses exactly the same name (same spelling) as the author who had first recognized the taxon and named it. This situation is clear: neither the *taxon* nor the *name* are new.

B. The writer uses a spelling slightly or totally different from that of the original name. Two possibilities again appear here.

1. The new spelling differs slightly (e.g. generally by one letter or a few letters) from the original spelling, and this difference in spelling is not intentional from the part of the writer : it may be due to a misspelling of his part (e.g. a mistake in copying the original text) or of the part of the printer (misprint). Such a spelling is an "incorrect subsequent spelling" and it has no status in nomenclature. In this case like in the preceding one, neither the *taxon* nor the *name* are new.

2. The writer uses intentionally a different name or spelling because he thinks, for some reason, that the original name created by the first author is incorrect or invalid and must be modified or replaced. In this case it is clear that the *taxon* is not new but that the *name* is. In modern times, this new name is called a new replacement name or nomen novum (ANONYMOUS, 1985 a), and it is generally presented with the indication "nom. nov.". In older times, it was sometimes presented as a new name in an explicit sentence, but in many other cases the fact that it was such a name must be inferred from other direct or indirect sources of evidence. This latter category may again be subdivided into two categories, although as will be discussed in detail below, no objective or reliable criteria currently exist to distinguish between them in all cases.

a. The new name is a *new replacement name* for the original name, i.e. a completely new name, not derived from the original one.

b. The new name is an *emendation* of the original spelling of the name, i.e. only the spelling of the name is changed but the new spelling is clearly derived from the original one. The current *Code* here distinguishes again two subcategories : *justified* emendations (which have no separate status in nomenclature) and *unjustified* emendations (which have a separate status in nomenclature).

Since in all the older texts (XVIII and most of XIX century texts) the above distinctions were often not clearly expressed in all words, we need some criteria to decide what kind of name was used by a given author. In this respect it will be useful to study separately several types of distinctions which may prove difficult in some cases : new genus name versus replacement name ; unjustified emendation versus incorrect subsequent spelling ; unjustified emendation versus new replacement name.

## NEW GENUS NAME VERSUS NEW REPLACEMENT NAME

SAVAGE (1986 : 259) states that according to the *Code* a name may be considered as a new replacement name only when it is "a new name *expressly* proposed as a replacement for an available name" (italics his). This is only partially correct. As a matter of fact, the words "proposed expressly" appear in the *Code* only in Art. 13(a)(iii), which deals with names published after 1930, but in Art. 12(b)(3), which concerns names published before 1931, the *Code* only states that "the proposal of a new replacement name (nomen novum) for an available name" is enough to make this name available. In the Glossary of the *Code* (p. 258), a "new replacement name", is defined as "A name established expressly to replace an already established name", which is more restrictive than the wording of Art. 12. In fact, one could discuss at length about the sense which should be given to the word "expressly" in these sentences, but this would make little sense. I have always believed that, to solve a problem, it is more useful to take time to think about it than to repeat or underline words. Now, when dealing with old taxonomic words, how will it be possible to distinguish between a new generic name and a new replacement name for an existing genus?

The writers of many older texts did not indicate the authors of the generic names they were using. For some well-known names (like *Rana*, *Bufo* or *Hyla*), this causes no problem, but the same is not true for less common names. Furthermore, a very special, but real, problem arises from the fact that LINNAEUS (1758, 1761, 1766, 1767) had a very wide concept of the genus, so that his genera were later subdivided. In many cases, the subsequent authors named the new genera with terms which LINNAEUS had proposed for species. For example, among the species placed by LINNAEUS (1758) in his genus *Rana* were *R. pipa*, *R. bufo* and *R. hyla*. LAURENTI (1768) used the same terms for his genera *Pipa*, *Bufo* and *Hyla*. In some cases however, this may raise some problems. For example, LINNAEUS's (1761) species *Rana bombina* served as the basis for OKEN's (1816) genus *Bombina*. MERREM's (1820) genus name *Bombinator* was obviously based on the same root. Is this latter name a new generic name or a new replacement name (or an emendation) for *Bombina*? This can be established only through a careful examination of the original publication. Fortunately, MERREM (1820) gave long and detailed synonymies in his book, and, not only did he not cite the generic name *Bombina* as a synonym of *Bombinator*, but, even more, he did not cite OKEN at all; he presumably did not know OKEN's (1816) *Lehrbuch der Naturgeschichte*. For this reason, I considered *Bombinator* Merrem, 1820 as a new generic name, not as a replacement name (or an unjustified emendation) for *Bombina* Oken, 1816 (D-26 : 13-14 ; D-29). This example shows that a careful analysis of the original texts is often required to ascertain the status of a name.

As a general rule, assurance that a new name is not the name of a new taxon but a new name for an existing taxon requires some evidence, in the original text, that this was the intention of the author. This evidence may be of various types, as will be shown by some examples studied in more detail below. The existence of such evidence answers to the qualification "proposed expressly" which appears in the *Code*.



## UNJUSTIFIED EMENDATION VERSUS INCORRECT SUBSEQUENT SPELLING

Here again, SAVAGE (1986) cites and italicizes the phrase “demonstrably intentional” without discussing its meaning in detail. However, the *Code* itself gives in its Art. 33(b)(1) additional information in this respect : “A change in the original spelling of a name may only be interpreted as “demonstrably intentional” when in the work itself, or in an author’s (or publisher’s) corrigenda, the original and the changed spelling are cited and the latter is adopted in place of the former, or when two or more names in the same work are treated in a similar way” (italics mine). Although this statement broadens already very much the concept of “demonstrably intentional” as compared to SAVAGE (1986), who seems to believe that this phrase may only mean “stated in full words”, and is enough to show that this author is wrong in considering names like *Bombuator* or *Calamites* as incorrect subsequent spellings or new generic names (see below), it is in my opinion still too restrictive and should be modified in future editions of the *Code*. As a matter of fact, the phrase “demonstrably intentional” cannot be restricted to the two possibilities mentioned in this sentence (either citation of both the original and the changed spelling, or a “similar” treatment of several names). These words mean : (1) that the new spelling was proposed in the clear *intention* to replace the original spelling of the name ; (2) that this intention was made clear by the writer in some way, not necessarily the only two ones mentioned above. One of these ways may be that the new spelling contains *by itself* this information. For example, when in older days a name was found to have been incorrectly formed with respect to its etymology (badly latinized, etc.), it often happened that a subsequent author changed the spelling in order to make it correct. Since in these remote times all biologists were cultured people, who knew Latin and Greek names or could easily trace them, it was often felt unnecessary to “expressly” write in all words that the name had been emended, because this was considered obvious.

In such cases, the evidence that the *intention* of the author was to replace a (supposedly) incorrect spelling by another one may be found in the new spelling itself, when it was correctly formed from the Latin or Greek root *whereas the original spelling was not*, or could be considered so. As we shall see in detail below, such a case clearly applies to the names *Megalophrys* versus *Megophrys*, *Bombuator* versus *Bombinator*, *Myiobatrachus* versus *Myobatrachus*, etc. If this is believed by some to be contrary to the wording of the *Code*, then this simply means that this wording is wrong, and should be corrected. If the ICZN decided that *Megalophrys*, *Bombuator* and *Myiobatrachus* are *not* unjustified emendations (but incorrect subsequent spellings having no status in nomenclature), because their authors did not write in full words that they were so, then I frankly declare it would be a stupid decision, because there can be no doubt in such cases about the *intention* of the authors of such spellings to correct the original names according to the Latin or Greek grammar.

This does not mean at all, as SAVAGE (1986) seems to believe, that I am ready to accept any subsequent spelling as an emendation. Some evidence that the *intention* to change the spelling must exist in the original texts, and in this respect it is useful to give an indicative, *non limiting*, list of such potential evidence. To be deemed an emendation, a name must meet at least either of the following conditions .

(C1) the name is presented in words as such,

(C2) both the original and the modified spellings are given, and the second one is retained by the writer as the valid one;

(C3) several names are treated in a similar way, e.g. corrected according to the etymology, or according to some, possibly arbitrary, rule which is evident from the context;

(C4) the modified spelling is clearly etymologically justified and correctly formed, while the original spelling was not, or could be considered not to be (see below for additional comments);

(C5) the modified spelling is introduced by the very author of the original spelling, either in a "corrigenda", as stated by the *Code* (which however does not precise if the latter must have been published in the same time as the original name, e.g. as an addenda to it, or may have appeared later), or in later publications, especially when the new spelling is used repeatedly in subsequent works and the original spelling definitively abandoned by the author;

(C6) the modified spelling is introduced by a subsequent author and used repeatedly in the same or, better, in subsequent works, whereas the original spelling is definitively abandoned by this author (especially when before introducing the new spelling he had made use of the original spelling in previous publications).

A few examples of these criteria shall be given and discussed below in the comment of some of SAVAGE's (1986) statements ; examples of the other ones may be given here.

Thus, the type (C3) of evidence may be illustrated by PALACKÝ's (1898) paper in which this author used a lot of emended spellings, according to a strange rule of his own, which is clearly evident in the text itself, although it is never stated in words : he systematically replaced the letters "ph", when they appeared *within* a generic name, by the letter "f" ; on the other hand, he did not modify the generic names which were *beginning* by "Ph". Strange and unjustified as this "rule" may be, PALACKÝ's (1898) action was clearly intentional and all these modified spellings are unjustified emendations, which have a separate status in nomenclature. Since only a few of these names have already been mentioned in recent works (DUBOIS, D-11, D-17, D-26 ; CLARKE, 1983), I am giving here in Table I a list of PALACKÝ's (1898) emendations of generic names, along with the original spellings of these names. Besides these, PALACKÝ (1898) also proposed a few emendation of specific names (*Rana cyanoflyctis*, p. 378 ; *Hyla stefeni*, p. 379 ; *Bufo filipinus*, p. 380 ; *Bufo dialofus*, p. 381).

An example of the type (C6) of criteria may be provided by the name *Ptychadaena* Parker, 1930. PARKER (1930 a) introduced the new spelling *Ptychadaena* without any explanation ; this spelling appeared twice in this paper, but the spelling *Ptychadena* Boulenger, 1917 was not mentioned. In his subsequent publications (e.g. 1930 b, 1932, 1936 a, 1936 b, 1937), PARKER always used the spelling *Ptychadaena*, but he never apparently mentioned the spelling *Ptychadena*, which he could not ignore, since he was well acquainted with BOULENGER's works. Despite the absence of any "explicit" statement in words in this respect, it is clear that PARKER (1930 a) intentionally modified the spelling *Ptychadena*, and that *Ptychadaena* must be considered an unjustified emendation, with its own status in nomenclature.

Now that we have discussed the major types of evidence for the fact that a new spelling was intentionally created by an author, it may be useful to see which are the principal clues to the reverse situation, i.e. to "incorrect subsequent spellings":

(C7) no explanation is given for the modified spelling, and the latter has no clear etymological justification (on the contrary, it may often be incorrectly formed while the original spelling was correctly formed);

Table I. – Unjustified emendations of generic names of amphibians created by PALACKÝ (1898).

Unjustified emendation	Page in PALACKÝ (1898)	Original generic name	Recent reference to PALACKÝ's (1898) name
<i>Amfignathodon</i>	375	<i>Amphignathodon</i> Boulenger, 1882	
<i>Asterofrys</i>	380	<i>Asterophrys</i> Tschudi, 1838	
<i>Calofrynus</i>	374	<i>Calophrynus</i> Boulenger, 1882, an unjustified emendation of <i>Kalophrynus</i> Tschudi, 1838	
<i>Chlorofilus</i>	374	<i>Chlorophilus</i> Baird, 1854	
<i>Cofofryne</i>	379	<i>Cophophryne</i> Boulenger, 1887	
<i>Eupemfix</i>	376	<i>Eupemphix</i> Steindachner, 1863	
<i>Genyofryne</i>	375	<i>Genyophryne</i> Boulenger, 1890	
<i>Glyfoglössus</i>	379	<i>Glyphoglossus</i> Günther, 1868	
<i>Megalofrys</i>	379	<i>Megalophrys</i> Wagler, 1830, an unjustified emendation of <i>Megophrys</i> Kuhl & Van Hasselt, 1822 a	D-17
<i>Nannofrys</i>	375	<i>Nannophrys</i> Günther, 1869	D-11
<i>Nectofryne</i>	374	<i>Nectophryne</i> Buchholz & Peters in PETERS, 1875	
<i>Pseudofryne</i>	374	<i>Pseudophryne</i> Fitzinger, 1843	
<i>Rhacoforus</i>	374	<i>Rhacophorus</i> Kuhl & Van Hasselt, 1822 a	D-11
<i>Rhombofryne</i>	378	<i>Rhombophryne</i> Boettger, 1880	
<i>Scafiopus</i>	381	<i>Scaphiopus</i> Holbrook, 1836	
<i>Scafiornina</i>	378	<i>Scaphiophryne</i> Boulenger, 1882	
<i>Stenofryne</i>	380	<i>Sphenophryne</i> Peters & Doria, 1878	

(C8) the original spelling is not mentioned, or, if it is, this is in a different part of the text, as a valid name, and no choice is made between both spellings;

(C9) the modified spelling appears only once in the text, and is never used again by the same author in subsequent publications, while this author may revert to the use of the original spelling in these papers;

(C10) the author of this modified spelling is known for having made numerous such mistakes (because of his carelessness regarding these matters, of his bad handwriting, which was difficultly read by the printer, of the carelessness of the printer, etc.).

A few examples here also may illustrate these general criteria.

Thus, besides the unjustified emendations mentioned above, PALACKÝ (1898) also introduced modified spellings which are certainly incorrect subsequent spellings according to type (C8) of evidence above, since he used in the same paper two, or even three, different spellings, including the original one, and did not choose between them: *Caluella*, p. 375, *Calluella* and *Calluela*, p. 380; *Megalxalus*, p. 375, and *Megalxcelus*, p. 378; *Batrachylodes*, p. 375, and *Batrachylodes*, p. 381. In a few other cases, he used generic or specific names which differ from the original names by no clear rule of transformation, which appear only once in the text, and which are therefore, according to criteria (C7), (C8) and (C9), best considered as incorrect subsequent spellings, without status in nomenclature (although their modification was possibly intentional in some cases): *Phrynomantes*, p. 376; *Grypnus* and

*Leuaporus*, p. 382 ; *Rana mascarenensis*, p. 377 ; *Ixalus kakhyensis*, p. 379 ; *Cofogyne sikimensis*, p. 379.

As I have already had the occasion to stress it (D-17), J.E. GRAY may be considered as a top specialist in the field of incorrect subsequent spellings : he often used different spellings for the same name in a given work, modified without any reason names previously proposed by other authors or even by himself, etc. For this reason, of the type (C10) of evidence, I suggested that the name *Megalophys*, which appears in GRAY (1842) and is considered by SHERBORN (1928) as an unjustified emendation of *Megalophrys* Wagler, 1830, is in fact an incorrect subsequent spelling, without status in nomenclature. In the same paper (D-17), I showed that the spelling *Megaphrys*, credited by SHERBORN (1928) to GRAY (1825), was also an incorrect subsequent spelling. These examples show that, contrary to what SAVAGE (1986) seems to believe, I have never considered all subsequent spellings as emendations, but based my opinion on a careful analysis of the texts themselves.

#### UNJUSTIFIED EMENDATION VERSUS NEW REPLACEMENT NAME

This problem is not tackled by SAVAGE (1986), but, for the sake of completeness, it will be briefly discussed here. A more complete discussion of this question may be found in a previous paper (D-29) ignored by this author.

The need for a distinction between these two types of names is a new one, which was created by the modifications introduced in Art. 32, 35 and 39 of the new edition of the *Code*. Before, both types of names had the same status, or rather, an unjustified emendation was only considered as a particular case within the general category of "new replacement name" (see e.g. D-11, D-17, D-18, D-26). As a matter of fact, as is shown in terms of logic in the dichotomic key presented above, these two types of names are very closely related, being only subdivisions of the category II(B)X(2) above : in both cases, a subsequent author replaces an existing name, which he deems to be incorrect or invalid for some reason, by a new replacement name. Of relatively minor importance in this case is the fact that this new replacement name may be "completely new", or obtained by a modification of the spelling of the original name. Furthermore, not only is this distinction of little relevance, but it is also, in many concrete cases, difficult to make, as is shown in more detail elsewhere (D-29).

The definition given by the *Code* (Art. 33) of an emendation ("any demonstrably intentional change in the original spelling of a name") does not give us any criterion to allow deciding in a clear, objective and indisputable manner, when one is confronted with a new spelling which "resembles" that of an already existing name, if it is an "intentional change in the original spelling" of the name, or a completely new name, based e.g. on a related but different etymology. When one thinks to this problem in some detail, one realizes that several criteria could be proposed for recognizing emendations (e.g.: given number of letters of difference with the original spelling ; changes in the radical or in the termination of the name ; recourse to a more exact etymology ; maintenance of the pronunciation of the name ; etc.), but that none of them is objective and general enough to account for all cases of emendations. The following examples, discussed in detail by DUBOIS (D-29), illustrate this complex problem : *Megophrys montana* Kuhl & Van Hasselt, 1822 and *Megophrys monticola* Smith, 1931 ; *Kaloula* Gray, 1831 and *Callula* Gunther, 1864 ; *Kassina* Girard, 1853 and *Cassina*

Cope, 1864 ; *Occidozyga* Kuhl & Van Hasselt, 1822 and *Oxydozyga* Tschudi, 1838 ; *Bufo* Laurenti, 1768 and *Batrachus* Rafinesque-Schmaltz, 1814 ; *Rana* Linnaeus, 1758 and *Ranana* Rafinesque-Schmaltz, 1814 ; *Hyla* Laurenti, 1768 and *Hylaria* Rafinesque-Schmaltz, 1814 ; *Triton* Laurenti, 1768 and *Triturus* Rafinesque, 1815 ; *Caecilia* Linnaeus, 1758 and *Cecilia* [Rafinesque-Schmaltz, 1814].

But, if the *Code* gives no precise clue for distinguishing emendation from nomen novum in these cases, why should this distinction be important? It only became important with the following new parts of Art. 32, 35 and 39 of the 1985 *Code*, which did not exist in the previous editions :

Art. 32(c)(iii): "An original spelling is an 'incorrect original spelling' if (...) in the case of a family-group name, it (...) is based (...) on an unjustified emendation of a generic name (...)"

Art. 35(d)(i): "A family-group name based upon an unjustified emendation of a generic name is an unjustified original spelling and must be corrected (...)"

Art. 39(a): "*Effect of unjustified emendations.* - If an unjustified emendation of the name of the type genus becomes itself the replacement name, the family-group name is then to be based upon it by correcting the name to the spelling required by the stem of the name of the replacement type genus ; the author and date of the family-group name remain unchanged."

At first reading, these new rules may appear trifling. In fact, as I have shown (D-29, D-41), in amphibians these modifications entail a change of spelling, author and date for one family-group name (*Cycloramphini* ; sole case noticed by SAVAGE, 1986, although on the basis of a wrong analysis, since he considers *Cyclorhamphus* as an incorrect subsequent spelling ; see below), and a change of author and date for 12 other family-group names (*Hylidae*, *Hylinae*, *Hylloidea*, *Megophryinae*, *Microhylidae*, *Microhylinae*, *Pipidae*, *Pipinae*, *Pipoidea*, *Ranidae*, *Raninae*, *Ranoidea* ; none of these changes is remarked by SAVAGE, 1986). Similar changes have been necessary in the higher nomenclature of chelonians (BOUR & DUBOIS, 1986) and will be necessary in all other groups. As justly stressed by LAURENT (1986 b), it is surprising that it was the ICZN, the function of which is to provide rules for allowing the greatest universality and stability possible in nomenclature, which introduced in 1985 these new rules which seriously threaten the stability of family-group names. Furthermore, these new rules are also liable to cause additional confusions and raise new problems even more difficult to solve, and for which rather artificial solutions must be found, if stability is to be preserved : see in this respect the difficulties raised by the new Art. 39(a) of the *Code* in the case of the family-group name *Triturinae* (D-29).

In conclusion, I repeat here my previously expressed opinion (D-29) that the new changes brought in Art. 32, 35 and 39 of the *Code* are not good and should be suppressed. I contend that unjustified emendations should still be considered as only particular cases of replacement names, and not be treated differently, even when they are the basis of family-group names. Furthermore, the particular treatment of these names suggested by the *Code* in such cases is contradictory with the fact that unjustified emendations continue to be considered by Art. 33(b)(iii) of the new *Code* as names having their own status in nomenclature and available with their own authors and dates.

On the other hand, if these modifications were to be maintained, then the ICZN *must* devise and publish precise criteria and rules to allow taxonomists to objectively decide if a given name is to be considered as an unjustified emendation or as a new replacement name. I predict that this would not be an easy task.

## WAGLER'S GENERIC REPLACEMENT NAMES

Now that we have seen the things rather generally, let us look more carefully at the problems raised by some of the generic names used by WAGLER (1830 b). SAVAGE (1986), following HOLTHUIS (1983), is mainly concerned with the problem of the name *Dendrobates*, because in my first paper on this question (D-18) I had presented an interpretation at variance with that accepted in all the previous applications published in the *Bull. zool. Nom.* on this case, one of which was even co-signed by SAVAGE (SILVERSTONE, 1971 ; MYERS & DALY, 1971 ; CUELLAR et al., 1972 ; PETERS et al., 1972 ; LESCURE, 1982). For some reason, maybe because both are members of the ICZN, HOLTHUIS (1983) and SAVAGE (1986) want to preserve this first interpretation, but unfortunately they are wrong. Because of the relative importance of this case as an exemplar one (since it has now been discussed by several authors and that no agreement seems to be currently reached among the supporters of opposed opinions), this question is dealt with here in some detail.

In WAGLER's times, the phrases "new replacement name" or "nomen novum", with the precise meaning they now have in the *Code*, did not exist. If the word "expressly" is taken literally (i.e. if these phrases must appear as such in the text), it is clear that no name published at the time of WAGLER, or even later, will ever qualify as a "nomen novum". But if it is taken, as suggested above, in the sense that the author must clearly show, somehow or other, that his intention is to propose a new name for an already existing taxon, and not for a new taxon, it will be clear that many names of this period qualify as such.

As I had already pointed out (D-18), the names proposed by WAGLER (1830 b) to replace already existing names are all presented in a similar way, with a footnote giving (1) the etymology of the new name, (2) the replaced name, with its author and sometimes its date and reference, and (3) sometimes, additional comments. What is important in my argumentation (D-18) is that I stressed this similarity of presentation and considered that all pairs of names which appear in this way in WAGLER's text (e.g., in *Amphibia*, *Asterodactylus* for *Pipa*, *Dendrobates* for *Hylaplesia*, *Enydriobius* for *Hylodes*, *Systema* for *Engystoma*, *Bombinator* for *Bombinator*) are to be treated similarly as a couplet composed of a replacement name and of a replaced name. Strangely however, neither HOLTHUIS (1983) nor SAVAGE (1986) discuss this aspect of my argument. Both of them refuse to consider *Dendrobates* as a replacement name for *Hylaplesia*, and SAVAGE (1986) further refuses to consider *Asterodactylus* and *Bombinator* as replacement names for respectively *Pipa* and *Bombinator*. None of them however discusses the cases of *Systema* or of *Enydriobius* (for which MYERS, 1962, and LYNCH, 1971, had already adopted the same interpretation as me).

SAVAGE's (1986) insistence on the fact that *Asterodactylus* is not a replacement name for *Pipa* is all the more incomprehensible to me that I had already stressed (D-21) that this name had first been proposed by WAGLER, not in his 1830 book, but in a 1827 paper, where he wrote : "(*Asterodactylus m. Pipa* Auctor)". To refuse to interpret such a presentation as a statement that the first of both these names is "proposed expressly as a new replacement name" for the second one makes really no sense, since this mode of presentation for new replacement names was very common in these times. To take examples dealing with amphibians, a similar presentation appears in the works of RAFINESQUE-[SCHMALTZ] (1814, 1815) and of GISTEL (1848) (see below). If SAVAGE refuses to consider *Asterodactylus* as a replacement name of *Pipa*, he should logically also refuse to consider e.g. *Triturus* Rafinesque, 1815

as a replacement name for *Triton* Laurenti, 1768, or *Philautus* Gistel, 1848 as a replacement name for *Orchestes* Tschudi, 1838 (see below). Since in these cases no diagnoses were given for the supposedly new taxa, and no included species mentioned, *Triturus* and *Philautus* should be considered as *nomina nuda*, without status in nomenclature. As a result, the valid name for the genus currently known as *Triturus* Rafinesque, 1815 should become *Molge* Merrem, 1820, and that for the genus currently known as *Philautus* Gistel, 1848 should become *Dendrobatorana* Ahl, 1927 (since, as will be shown elsewhere [MORÈRE & DUBOIS, in preparation], the holotype of *Hylambates dorsalis* Peters, 1875, type species of *Dendrobatorana*, belongs in fact to the genus *Philautus*). Numerous similar examples could easily be found. SAVAGE (1986) is not embarrassed with these problems, since he does not at all consider the consequences of his refusal to recognize that *Asterodactylus* was clearly proposed as a replacement name for *Pipa*.

One may wonder why WAGLER felt necessary to replace some existing generic names by others, coined by him. Let us note in passing that even if we had no clue to help us to understand why he did so, that would not in the least allow us to reject the evidence, and in this respect the discussion given by SAVAGE (1986) about the name *Cacopus* is completely irrelevant. no matter if GÜNTHER (1864) was wrong in believing the name *Uperodon* to be preoccupied, the fact is that he clearly and "expressly" presented *Cacopus* as a replacement name for the latter! However and fortunately in this case, WAGLER himself gave us the explanation. To be sure, to find this it is necessary to read Latin and German in WAGLER's text, but I am of the opinion that it is not possible to deal correctly with nomenclatural problems when one is unable to read, or at least decipher, Greek, Latin, German and French!<sup>1</sup>

WAGLER (1830 b : 17) gives us a first indication in a footnote concerning the mammalian generic name *Tapirus*, for which he proposes the new replacement name *Rhinochoerus*. He then quotes LINNAEUS, as follows : "Nomina generica, quae ex graeca vel latina lingua radicem non habent, rejicienda sunt Linné Philos. bot. stud. Spreng. p. 265." (Generic names, which do not have Latin or Greek roots, must be rejected). It is thus clear that he rejected as invalid all *generic* (not *specific*) names for which he did not find a root in Latin or Greek languages. Careful examination of the whole book of WAGLER bears this interpretation out : in all those cases in which an existing generic name was neither Latin, nor Greek,

1 One of the readers of the manuscript of this paper crossed this paragraph out, writing "Horrors!" in the margin and adding, "My God, you even demand that people read French! It may therefore be interesting to point out here that, if English is certainly, at the moment, the most "international" of all languages, French is the second one in this respect. According to MALHERBE (1983), the ten most "international" languages rank as follows: (1) English, official or "privileged" language in 47 countries; (2) French, 26 countries; (3) Arab, 21 countries; (4) Spanish, 19 countries; (5) Portuguese, 7 countries; (6) German and Swahili, 5 countries; (8) Dutch, 4 countries; (9) mandarin Chinese and Italian, 3 countries. The six official languages of the United Nations are English, French, Spanish, Russian, Arab and mandarin Chinese. If the number of persons in the world who speak a given language as their mother tongue is considered, English, with 320 millions, is only second, after mandarin Chinese (700 millions) and before Hindi Urdu (280 millions), Spanish (190 millions) and Russian (160 millions).

In fact, in one disagrees with my demand that zoologists should be able to "read, or at least decipher" French, this simply means that only one language in the world, English, is considered worthy of being known by them! I would not support such a statement, especially when one deals with disciplines, like systematics and nomenclature, in which old texts of the XIX century and even before are still in current use. In these times the leadership of English in science was not established, and many important works were written in French, German, Latin and other languages. Nowadays, if 67% of the scientific publications in the world are in English, 8% are in French, which cannot be considered trivial. In recent years, other languages, especially Spanish, have had a growing use in some fields, like zoology, and there is no reason to believe that this movement will stop. Ignorance of all the non English scientific literature cannot be profitable to any scientist in the world, since ignorance has never been useful to anyone.

nor based on Latin or Greek roots, WAGLER proposed a new replacement name. In some cases, he even expressed his perplexity as to the possible etymology of a generic name, which for this reason he replaced by a nomen novum. Thus, the name *Systema* is proposed to replace *Engystoma*, which is presented as follows in a footnote on p. 205: "Gen. *Engystoma* (quid?) *Fitzing*."

After WAGLER's death in 1832, MICHAELLES (1833) reproduced in *Isis von Oken* an unpublished manuscript of WAGLER, which had been written before the 1830 book of this author. In this text we can find additional explanations of the reasons why WAGLER rejected some generic names as invalid. In a footnote (in MICHAELLES, 1833 : 888), WAGLER states that he found no evidence of the use of the names *Hyla* and *Calamita* in classical Latin texts, and that for this reason these names must be rejected. He states that he found the name *Calamites* in PLINIUS, as the name of the common European treefrog. He adds that unfortunately FITZINGER had already given the name *Calamita*, "(das in *Calamites* umgeändert werden muß)" (which must be changed into *Calamites*) to another genus of frogs, and that, in order not to upset FITZINGER's work, he refrains from using the name *Calamites* for the European treefrog. In consequence he proposes the new replacement name *Discodactylus* for *Hyla*, the latter not being of classical Latin or Greek origin. This footnote clearly shows : (1) that, as a general rule, WAGLER did propose new replacement names for generic names considered by him invalid, because they were not of strictly classical Greek or Latin origin (i.e. a name used as such in classical Latin or Greek, or a name based on classical Latin or Greek roots) ; (2) that nevertheless WAGLER made clearly the distinction between new names for already known taxa, and for new taxa, and that he respected the works of previous authors (his refusal to use the name *Calamites* for a different genus than that called *Calamita* by FITZINGER – after SCHNEIDER, 1799 – is a rare example, for this epoch, of respect of the works of others) ; (3) more specifically, that SAVAGE's (1986) analysis of the status of the names *Calamites* and *Dendrobates* is in error.

This leads us now to discuss the case of the unjustified emendations of generic names proposed by WAGLER (1830 b), such as *Bombinator*, *Megalophrys* or *Calamites*. The name *Calamites* is expressly presented by WAGLER (in MICHAELLES, 1833) as a replacement name for *Calamita*, and could either be considered as a nomen novum or as an unjustified emendation of this latter name (see discussion above). Strangely, SAVAGE (1986) considers it as the name of a new genus, overlooking the fact that WAGLER (1830 b : 200) expressly wrote : "CALAMITES (...) *Fitzing*.", thus clearly acknowledging FITZINGER as the author of the taxon, even though he emended his name. In reality, in SAVAGE's logic, if *Calamites* is not accepted as a replacement name or an unjustified emendation for *Calamita*, it should not be considered as the name of a new genus (since it is credited to a previous author), but as an incorrect subsequent spelling, without status in nomenclature. It is true that the spelling *Calamita* does not appear in WAGLER's (1830 b) book, but it appears in the manuscript of WAGLER, anterior to this 1830 book, which was later published by MICHAELLES (1833). I contend that this case is a typical example of type (C3) of evidence for emendations presented above, which is also acknowledged in Art. 33(b)(1) of the new Code.

SAVAGE's (1986) too rapid way of working has another, quite funny indeed, consequence. SAVAGE (1986) states that *Calamites* is not an unjustified emendation of *Calamita*, but the name of a new taxon. He adds that FITZINGER's (1843) designation of *Hyla cyanea* as type species of *Calamites*, rediscovered by DUBOIS (D-26 : 19), is valid, but he does not



discuss the consequences of this fact : these would be that *Calamites* Wagler, 1830 would be a senior synonym of the well-known and much used generic name *Luoria* Tschudi, 1838! Fortunately, *Calamites* Wagler, 1830 is preoccupied by *Calamites* Guettard, 1770 (see D-26 . 14), so that, even if SAVAGE (1986) was right, no nomenclatural disruption would result. However, these facts and their consequences should all be considered and discussed when such question are tackled : this clearly shows that in nomenclature a very slight divergence of opinion as to the status of a name may have considerable consequences in the long run.

As for the name *Bombuator*, SAVAGE (1986) is still less excusable to consider it as the name of a new genus, because WAGLER (1830 b) mentions the name "*Bombinator* Merr." in a footnote on p. 206 and in the index on p. 346. In Latin, the name "*bombuator*" (bee) only did exist, while the names "*bombina*" and "*bombinator*" did not exist but were coined by XVIII and XIX century authors on the basis of the name "*bombus*" (buzzing). That *Bombuator* cannot either be considered as a fortuitous incorrect subsequent spelling is also shown by the fact that this name alone (and not *Bombinator*) appears on several occasions in WAGLER's book (pp. 132, 206, 294, 296, 301, 302, 303, 305, 306, 346). It is thus clear that the spelling *Bombuator* was purposely used by WAGLER instead of the spelling *Bombinator*, but for the same taxon as MERREM (1820). If this is not an unjustified emendation, I wonder which name of this epoch will qualify for this category.

To come back finally to the name *Dendrobates* now, it was clearly presented by WAGLER (1830 b) as a replacement name for *Hylaplesia* because the latter name was not acceptable according to WAGLER's conceptions, being based on the root "*Hyla*" which was rejected by WAGLER (1830 b ; in MICHAELLES, 1833) as invalid since it did not exist in classical Latin. The fact that WAGLER (1830 b) only recognized some of the species originally included in *Hysaplesia* (and *Hylaplesia*) by BOIE (in SCHLEGEL, 1826, 1827) is of no relevance here, because, as remembered above in the dichotomic key under the heading II, an author may perfectly, while accepting a taxon created by a previous author and crediting it to him, modify in part the diagnosis or content of the taxon : by doing so he does not create a new taxon, because otherwise any new modification of the diagnosis or content of a taxon would result in the creation of a new taxon and of a new name, and no stability of taxonomy and nomenclature would ever be possible.

As for the status of the name *Hylaplesia* itself, it is certainly open to discussion. It might be possible to consider it very formally as an incorrect subsequent spelling. However, as I have shown (D-18, D-21), this name has an etymological justification (being based on *Hyla*), while *Hysaplesia* has none and is clearly the result of a misprint for *Hylaplesia*. Since the spelling *Hylaplesia* has been used by various authors since its creation, while the spelling *Hysaplesia* has remained ignored until the paper by STEJNEGER (1937) where it was resurrected, this name, on which is based the family-group name Hylaplesidae, is better considered as having an independent status in nomenclature (for more detailed discussions of other similar cases, see D-17). It is therefore justified to consider *Hysaplesia* Boie in SCHLEGEL, 1826, which was in reality clearly a misprint in the original publication, as the "correct original spelling" of the name in the sense of the Code, and *Hylaplesia* Boie in SCHLEGEL, 1827 as an unjustified emendation of the latter. Finally, *Dendrobates* Wagler, 1830 is without possible doubt a new replacement name for *Hylaplesia*. Therefore the problems raised by DUBOIS (D-18) concerning the validity of the names *Dendrobates* and Dendrobatidae remain, and must be solved by an action of the ICZN.

In conclusion the generic names *Asterodactylus*, *Dendrobates*, *Enydrobius*, *Systema*, *Bombinator*, *Calamutes* and *Megalophrys* are all new replacement names (or, if one prefers, unjustified emendations, for the last three ones) for existing generic names which were believed by WAGLER (1827, 1830 b) to be invalid because they were not of strict classical Latin or Greek origin. They have therefore by definition the same type species as the replaced names, and SAVAGE's (1986) analysis is in error.

#### OTHER PROBLEMS RAISED BY SAVAGE (1986) IN GENERIC NAMES

(1) *Cacopus* Gunther, 1864. - As already tackled above, the analysis of this case presented by SAVAGE (1986 : 261) is clearly in error. He states that *Cacopus* is a replacement name for *Hyperodon*, "an incorrect subsequent spelling (which has no status) as *Uperodon* is an available name". SAVAGE (1986) clearly mingles two very different things : the fact that an author has intentionally introduced a new spelling (emendation) or a new name (nomen novum), because he *believes*, for some reason, that the original name is incorrect or invalid ; and the fact that this action is or not *justified* according to our current rules. It is perfectly true that *Uperodon* Duméril & Bibron, 1841 is an "available name" and that, according to the 1985 *Code* (and also to the preceding editions), it does not have to be emended. But this has no bearing on the question whether GÜNTHER (1864) intentionally decided to replace it! As a matter of fact, GÜNTHER (1864 : 415) did clearly and explicitly state that he did so and why : "The correct spelling of this word would be *Hyperodon*, a name long previously given to a genus of Cetaceans. Besides, these frogs have no vomerine teeth." This means that : (1) GÜNTHER (1864) considered (in error, but this needs not concern us here) the spelling *Uperodon* to be invalid because incorrectly formed, in his opinion, from the Greek root ; (2) he stated that the correct spelling of this name should be *Hyperodon* ; (3) but he immediately added that this latter name was also invalid, being preoccupied ; (4) furthermore, he added that this name was inappropriate for the frogs in question, because it did not correspond to the real biological properties of these animals ; (5) consequently, he proposed the replacement name *Cacopus* for this genus. Only two interpretations of this case are possible. The first one, which I had proposed (D-26), is simply to consider *Cacopus* as a replacement name for *Uperodon*. As a matter of fact, (1) GÜNTHER clearly considered this latter name as invalid, and (2) he only mentioned the spelling *Hyperodon* as the potential correct spelling of this name, but not as a valid name. However, if one wishes strictly to follow the very formal proposal of SAVAGE (1986) that *Cacopus* be considered a replacement name for *Hyperodon*, then the latter name must be considered an unjustified emendation of *Uperodon*, that must be credited to GÜNTHER (1864), but cannot in the least be viewed as an incorrect subsequent spelling! This much more formal solution, although it does not change the final result, is less logical, because it credits GÜNTHER (1864) with a name which he himself rejected in the very work where he created it.

(2) *Philaustus* Gistel, 1848. - Concerning this name, SAVAGE (1986 : 261) writes : "I have not seen the original descriptions (sic) of the latter name but suspect it does not qualify as a new replacement name." This statement sounds quite strange to me.<sup>2</sup> Personally, when I

2. I am even surprised that the journal *Copeia* should have published this sentence

have not seen a publication, I either try to obtain and see it by myself, or, when this proves impossible, I use second-hand information. It is true that such information may sometimes be in error (and this is the reason why I always try my best to obtain the original paper), but, at least, it is better than no information at all. SAVAGE (1986) seems to act differently. He apparently does not try seriously to see the original publication (for if he had, he certainly could have succeeded, for example by writing, as I have done in this case, to the British Museum library), and he a priori mistrusts the information given in a recent paper by a colleague, who furthermore stated that he had *seen* the original paper (since, as is easy to verify, I always state in the bibliographies of my papers when I have *not seen* a given publication). This is a strange attitude. Disappointing as it may be for SAVAGE, I must precise that here again he is in error. To avoid repetition of this error, I am obliged to give here more details about GISTEL's (1848) work. That may be useful to all batrachologists.

In the introduction of his book, GISTEL (1848 : viii-xi) presents a very long alphabetical list of generic names, for most of which he proposes new replacement names. The way names are presented may be illustrated by the following example, at the beginning of the list : "*Acanthoderus* (Palisot de Beauvois. Wanze : *Lygaeus sanctus*) bleibt ; aber *Acanthoderus* (Serv. Cerambycid.) heißt : *Scamillus* (Nob.)." This means that the first genus name cited *stands* (bleibt), while the second one is replaced by another one, and is (*re*)named (heißt) *Scamillus*. The words "bleibt" and "heißt", which appear in the first examples, are either abbreviated ("bl." for "bleibt") or omitted ("heißt") in the following of the text ; the word "Nobis" is abbreviated first in "Nob.", then in "N.". Other replacement names are also proposed in a slightly different manner in footnote 19 of p. xi.

As far as the name *Phylautus* is concerned, it appears as follows on p. x of GISTEL (1848) : "*Orchestes* (Illig. Kaf.) bl. - *Orchestes* (Tschudi, Isis 1838. 853.) : *Phylautus*, N. - *Orchestes* (Costa Cenni, Crustac.) : *Encopsis*, N." This is a clear indication that *Phylautus* Gistel, 1848 is proposed as a new replacement name for *Orchestes* Tschudi, 1838, because the latter is preoccupied by *Orchestes* Illiger, 1798 (and by *Orchestes* Leach, 1830) (see D-11).

In view of the rarity of GISTEL's (1848) text, I felt it useful to prepare and give here a complete list of the replacement names proposed in this book for amphibian genera (the same work should also be done for other groups), most of which have not been recently cited (see Table II). These names should be included in the synonymies of their respective genera, as was done by DUBOIS (D-11) in Ranoidea. Apart from *Phylautus*, the only one of these names to have been considered to be valid in the recent years is *Hydromantes* ; however this name was recently shown to be invalid (D-24).

(3) *Astroductylus* [Hogg, 1838]. - Contrary to SAVAGE (1986), this name is an unjustified emendation of *Asterodactylus* Wagler, 1827, not an incorrect subsequent spelling, by virtue of criteria (C2), (C4) and (C6) above. As a matter of fact, both this spelling and the original one have an etymological justification : *Astroductylus* is based on the Latin word *astrum*, derived from the Greek *ἀστρου*, while the original spelling *Asterodactylus* was based on the Latin word *aster*, derived from the Greek *ἀστῆρ*. Both *astrum* and *aster* mean "star", and this refers to the shape of the extremities of the fingers of these animals. Besides, in all his papers, HOGG (1838, 1839 a, 1839 b, 1841) always used the spelling *Astroductylus* (and the derived spelling *Astroductylidae*) for the name of these animals, and never reverted to the original spelling *Asterodactylus*. However HOGG was clearly aware of the original spelling, which he

Table II. - New replacement names for generic names of amphibians created by GISTEL (1848).

New replacement name	Page in GISTEL (1848)	Original generic name	Recent reference to GISTEL's (1848) name
<i>Baryboas</i>	xi	<i>Pelophilus</i> Tschudi, 1838	
<i>Borborocottes</i>	xi	<i>Palaeobatrachus</i> Tschudi, 1838	
<i>Bradytes</i>	xi	<i>Bradybates</i> Tschudi, 1838	
<i>Buccinator</i>	xi	<i>Boophis</i> Tschudi, 1838	D-11
<i>Cotobates</i>	xi	<i>Hemidactylum</i> Tschudi, 1838	
<i>Dendricus</i>	viii	<i>Buergeria</i> Tschudi, 1838	D-11
<i>Dendromanus</i>	xi	<i>Microhyla</i> Tschudi, 1838	
<i>Dendromedusa</i>	xi	<i>Hylaplesia</i> BOIE in SCHLEGEL, 1827	
<i>Epipole</i>	ix	<i>Eucnemis</i> Tschudi, 1838	D-11
<i>Hydromantes</i>	xi	<i>Geotriton</i> Bonaparte, 1832	D-24
<i>Hydroscoptes</i>	xi	<i>Pseudosalamandra</i> Tschudi, 1838	
<i>Limnarches</i>	xi	<i>Ambystoma</i> Tschudi, 1838	
<i>Pelida</i>	xi	<i>Hyladactylus</i> Tschudi, 1838	
<i>Pelodytes</i>	xi	<i>Pseudotriton</i> Tschudi, 1838	
<i>Philautus</i>	x	<i>Orchestes</i> Tschudi, 1838	D-11, D-26
<i>Phyllodytes</i>	xi	<i>Cornufer</i> Tschudi, 1838	D-11
<i>Polyphone</i>	xi	<i>Ranoidea</i> Tschudi, 1838	
<i>Pyleus</i>	xi	<i>Pseudobufo</i> Tschudi, 1838	
<i>Trutogenus</i>	xi	<i>Andrias</i> Tschudi, 1837	
<i>Troglobates</i>	xi	<i>Palaeophrynos</i> Tschudi, 1838	
<i>Xiphocotinus</i>	xi	<i>Xiphonura</i> Tschudi, 1838	
<i>Zoodiotes</i>	xi	<i>Hylarana</i> Tschudi, 1838	D-11

cited when he commented (HOGG, 1839 a : 268) on WAGLER's (1830 b) book where this spelling appears. There can therefore be no doubt about the intentionality of his act.

(4) *Lophohyla* Miranda-Ribeiro, 1926. - Quite funnily, SAVAGE (1986 : 261) writes concerning this name : "*Lophuhula* (sic) Miranda-Ribeiro, 1926 is an incorrect subsequent spelling." In reality, the spelling which appears in MIRANDA-RIBEIRO's (1926) work is not *Lophuhula*, but *Lophohyla*. This name must be considered an unjustified emendation of the original spelling *Lophyohyla* (for more detail, see D-26 : 21-22), by virtue of criterion (C5): this name appears several times in MIRANDA-RIBEIRO's (1926) work, and again once in a subsequent paper (MIRANDA-RIBEIRO, 1937), while the original spelling of the names does not appear any more in these texts. This name is clearly based on *Hyla*, but the etymology of the element *lopho* is unclear to me : it seems to be derived from the Greek λοφιά (mane), which does not make sense to me. However, there can be no doubt about the intention of MIRANDA-RIBEIRO to use the spelling *Lophohyla* for this genus, and the spelling must be considered an unjustified emendation. As for the spelling *Lophuhula*, cited by SAVAGE (1986), it is a new spelling. I admit however perfectly that it is to be considered as an incorrect subsequent spelling, without status in nomenclature, just like the three other new and incorrect spellings which appear in various parts of his paper (*Cycloramphus*, Phrynobatrachinae, *Petropedetinae*).

(5) *Myiobatrachus* [Bonaparte, 1850]. - Contrary to SAVAGE (1986), this name must be considered an unjustified emendation of *Mvobatrachus* Schlegel, 1850, not an incorrect sub-

sequent spelling, by virtue of criteria (C4), (C5) and (C6). As a matter of fact, both this spelling and the original one have an etymological justification : *Myobatrachus* is based on the root *myo-*, derived from the Greek *μῦς, μῦς* (rat, mouse ; muscle) ; *Myiobatrachus* is based on the root *myio-*, derived from the Greek *μύια* (fly). Besides, BONAPARTE (1850, 1852 b) used the spellings *Myiobatrachidae* and *Myiobatrachina* in two different papers (see D-26 : 22), and the spelling *Myiobatrachus* was accepted as valid by the very author of the original name *Myobatrachus*, SCHLEGEL himself (1858).

(6) *Cyclorhampus* Agassiz, 1846. - This spelling is considered by SAVAGE (1986) as an incorrect subsequent spelling of *Cycloramphus* Tschudi, 1838, but it is in fact an unjustified emendation of this latter name, as shown by types (C2) and (C4) of evidence. In TSCHUDI'S (1838) text, this generic name was spelled *Cycloramphos* on p. 41 and *Cycloramphus* on p. 81. AGASSIZ (1846 : 110) cites both these spellings, and, in front of each of them, writes "*C-rhampus*" between parentheses. This is a clear indication that he considered *Cyclorhampus* as the only justified spelling for this name, which should in his opinion replace both original spellings. The reason for that is clearly the recourse to the etymology, this name being based on the Greek word *ῥάμφος* (beak), which is correctly latinized into *rhamphus*.

(7) *Alytes* Wagler, 1829. - For this name, SAVAGE (1986 : 261) most strangely writes : "the type species of *Alytes* Wagler, 1829, is *Bufo obstetricans* Laurenti (sic), 1768, by indication (Art. 12b.5)". This is perfectly true, but by no means contradictory to what I had written (D-26 : 18), which SAVAGE does not seem to have understood. I therefore give here a translation of the relevant part of my discussion on this question : "In the first text where the generic name *Alytes* appears (WAGLER, 1829 : 70), this name is accompanied by no description or diagnosis, but is presented in the combination *Alytes obstetricans*. The name *Bufo obstetricans* Laurenti, 1768 being at this epoch the only existing specific name which could apply to this taxon (see SHERBORN, 1902 : 682), one may consider that the name *Alytes* is available as from 1829, with *Bufo obstetricans* Laurenti, 1768 for type species by monotypy. One could also consider this indication as insufficient, the name *obstetricans* not being defined by the mention of his author and date : in this case, *Alytes* would be a nomen nudum in WAGLER (1829), and would only obtain a status in nomenclature with the publication by WAGLER (1830 a : [53], pl. XXII) of a description and of figures showing the species *Alytes obstetricans* (Laurenti, 1768)." (D-26 : 18). My concern here was whether the specific name *obstetricans* mentioned by WAGLER (1829) was to be considered as a new name, which would then be a nomen nudum, or as a subsequent use of the specific name *Bufo obstetricans* Laurenti, 1768. It is true that the subsequent publications of WAGLER (1830 a, 1830 b) demonstrated without ambiguity that it was this latter name which had been used by WAGLER (1829), but, had these subsequent works never appeared, there would be no evidence at all in the original paper itself (WAGLER, 1829) for this interpretation. The only evidence which could be used at this date was the fact that *Bufo obstetricans* Laurenti, 1768 was the only specific name *obstetricans* which existed then in the whole animal kingdom. These are subtle matters, I admit, but which should not discourage a truly serious student of nomenclatural problems.

(8) "*Elosia nasuta* Tschudi, 1838". - This is the last of the examples cited by SAVAGE (1986 : 261) to demonstrate that "it behooves any user of Dubois' paper to review the original literature to determine if a particular name is an emendation or an incorrect spelling." While I am not embarrassed by this suggestion, I think this example clearly shows that the

batrachologists interested in suprageneric nomenclature of amphibians should consult my work directly, rather than SAVAGE's (1986) comments upon it, which are based on an approximate (and sometimes completely erroneous) understanding of its content. As a matter of fact, SAVAGE (1986 : 261) writes : "Other corrections that need to be mentioned regarding the genera discussed by Dubois are : (...) d) the name *Elosia nasuta* Tschudi, 1838, is an incorrect subsequent spelling." Now, here is the exact translation of my own comment on this question : "GORHAM (1966 : 111) and LYNCH (1971 : 166) believe that there exists a nominal species *Elosia nasuta* Tschudi, 1838. In fact, the binomial *Elosia nasuta* which appears in TSCHUDI's (1838 : 77) work is clearly a new combination proposed for the nominal species *Hyla nasus* Lichtenstein, 1823. The spelling *nasuta* used by TSCHUDI (1838) for the specific name cannot be held to be an unjustified emendation of the name *nasus* Lichtenstein, 1823, but must be regarded as an incorrect subsequent spelling, devoid of nomenclatural status (the same is true for the spelling *nasulus*, which also appears in TSCHUDI's 1838 work)." I think this needs no additional comment.

#### VALID FAMILY-GROUP NAMES

While SAVAGE (1986) tends to favor a very strict, restrictive and even rigid conception of the *Code* regarding the status of new replacement names, he adopts the reverse attitude concerning the validity of family-group names. In this case he clearly praises a return to pre-*Code* rules, since he does not admit the principle or priority for these names. As a matter of fact, he challenges the use of some family-group names which I consider valid on the basis of this principle. In particular, he suggests that Dactylethrinae Hogg, 1838 should be replaced by Xenopodinae Fitzinger, 1843, and that Tornieriobatinae Miranda-Ribeiro, 1926 should be replaced by Nectophryninae Laurent, 1942. He seems also, though less clearly, to suggest that Petropedetinae (which he qualifies as "widely used") should be given priority over Phrynobatrachinae. Although this point is only slightly tackled by SAVAGE (1986) in this paper, it is of importance, because it is clear that SAVAGE's opinion in this respect was accepted or shared by the editors of *ASW*, and one may in fact find in SAVAGE's (1986) paper the "theoretical" justification for some of the decisions taken in *ASW* concerning the valid names of some family-group taxa, which I already commented upon elsewhere (D-41).

As I already pointed out (D-26 : 6), MYERS & LEVITON (1962 : 290) rightly stressed that one of the causes of instability of familial nomenclature in zoology stems from the fact that several conceptions have existed as to the rules which should be followed to determine the valid name of a family-group taxon. Thus, according to the various "rules" followed in the past by different authors, the name retained as the valid one may have been : (R1) the family-group name based on the *oldest available genus-group name* belonging to the family-group taxon in question ; (R2) the family-group name based on the *oldest valid genus-group name* of the family-group taxon in question ; (R3) the *first family-group name* to have been used with a *correct spelling* (suffix in -idae or -inac) ; (R4) the *oldest family-group name* ever proposed for this group, even when it was then incorrectly formed (suffix in -ae, -ida, -ina, -ides, etc.). The experience shows that the use of rules (R1) and (R2) leads to catastrophic consequences as to the stability of family-group names. As a matter of fact, under such rules, very limited taxonomic changes may lead to a change of name for the family-group taxon :

in case (R1) this may be caused by the simple addition to or subtraction from this taxon of a genus the name of which is older than all the other generic names of the group ; in case (R2) this may be caused by the simple fact that a generic name, once considered valid, be rejected as a junior homonym or (subjective or objective) synonym, or on the reverse retired from synonymy. Rule (R3) would not do justice to the fact that in older times there was no rule as to the correct spelling of family-groups names and that nevertheless authors of this epoch, although using spellings which are currently considered incorrect, clearly created family-group names and should be credited with the authorship of these names. Rule (R4) is the one that ensures the highest stability possible for family-group names and credits these names with their proper authors. As I had stressed in my 1984 paper (D-26), the rules then in force according to the second edition of the *Code* (ANONYMOUS, 1964, 1974) were excellent, because they allowed a maintenance of the stability of family-group nomenclature even when minor changes were introduced in the generic content of a family-group taxon or in the status of some generic names. Unfortunately, as commented above in detail, the changes brought to Art. 32, 35 and 39 in the 1985 *Code* are highly open to criticism and should in my opinion be reconsidered.

Some comments are necessary here concerning Art. 40 of the *Code*, which is called upon by SAVAGE (1986) and also in ASW to justify certain decisions taken by these authors. The relevant parts of this Article are as follows :

"Article 40. Synonymy of the type genus. -

(a) *After 1960.* - When, after 1960, the generic name on which a valid family-group name is based is rejected as a junior synonym, that family-group name is not to be replaced (...).

(b) *Before 1961* - If a family group name has been replaced before 1961 because of such synonymy, and the replacement name has won general acceptance, it is to be maintained.

(..)

(ii) in the event of divergent interpretations of the expression "general acceptance", the case is to be referred to the Commission for a decision." (ANONYMOUS, 1985 a : 81)

Some comments on this article seem appropriate here. First of all, it is clear that this article was devised in order to limit the nomenclatural disturbances which would be entailed by a too strict application of the principle of priority in the family-group, since in the past many authors in fact had followed the rule (R2) above. However, this partial suspension of the principle of priority to family-group names is limited in time, since it applies to actions prior to 1961. The aim of setting this limiting date is clear : it is to ensure that, from 1961 on, the principle of priority strictly applies to family-group names, as well as to genus-group and species-group names. It should therefore be clear that this article only applies to family-group names which have been proposed and have won general acceptance before 1961, and cannot even be considered as potentially applicable to names either proposed or generally accepted after that date. Thus for example this article cannot in the least be called upon to conserve the name *Ichthyophoridae* Taylor, 1968, which was proposed after 1960 and is a junior synonym of *Epicriidae* Fitzinger, 1843. It could not either be called upon to conserve *Bombininae* Fejerváry, 1921, which has been used only once before 1961 (D-37) and is a junior synonym of *Bombinatorinae* Gray, 1825, a name which has had a large use in the XIX century, and is the valid name of the subfamily, if one wishes to recognize it as a distinct taxon (D-19, D-26, D-29, D-35, D-39).

A second comment is that Art. 40 only applies to the case where a family-group name, based on a generic name rejected as a junior synonym, is replaced by a family-group name based on the generic name which is the senior synonym of the rejected generic name (as in the example given in Art. 40 of the *Code*). Thus this article could not apply in the case of *Petropedetinae* versus *Hemimantinae*, even if, which is far from being true, the name *Petropedetinae* had "won general acceptance" before 1961.

Finally, it is important to note that in the case of disagreements between different authors as to the fact that a name has or not "won general acceptance", the ICZN alone is enabled to take a decision. In other words, in such cases it is the responsibility of those who believe, contrary to others, that Art. 40 may be called upon to conserve a more recent name, to submit an application to the ICZN and give evidence that this recent name has "won general acceptance". Unless they do it, the principle of priority must apply in these cases as in general.

The proposals of SAVAGE (1986) may now be considered in the light of these comments.

The case of *Dactylethrinae* versus *Xenopodinae* was already discussed elsewhere (D-41) I contend that the name *Xenopodinae* was only very rarely used before 1961, and even before 1986, and that it cannot be considered as having "won general acceptance". The name *Dactylethrinae*, which has been more largely used than this latter name and which has priority, should be conserved for this taxon. I feel it useless to discuss this problem in more detail here and to give lists of bibliographic references in support of my assertions as long as nobody deems it justified to submit officially this problem to the ICZN.

The case of *Petropedetinae* was also discussed elsewhere (D-41). SAVAGE (1986) qualifies this name as "widely used", but does not produce evidence for this statement : as shown elsewhere (D-41), I can list 11 references to use of this name, versus 35 references to use of *Phrynobatrachinae*. Furthermore, Art. 40 cannot be called upon in this case, for the reasons developed above, and an action of the ICZN is necessary here if the name *Hemimantinae* is to be rejected.

Finally, the most striking case is that of the names *Tornieriobatinae* and *Nectophryninae*. SAVAGE (1986 : 261) writes : "Art. 40b is marginally applicable here since the subfamily has been rarely recognized, nevertheless I would favor use of *Nectophryninae*." Actually, it is misleading to say that the subfamily "has been rarely recognized", since I personally know only two publications where it was recognized before my own works (D-14, D-19, D-26, D-29, D-35) : that of MIRANDA-RIBEIRO (1926) where he created the name *Tornieriobatinae*, and that of LAURENT (1942) where he created the name *Nectophryninae*! This latter author himself seems to have never used again this name, or recognized this subfamily, in subsequent publications. Even if I missed a few papers where these names were used, it is clear that it is impossible to speak in such a case of "general acceptance" before 1961, or even before 1986. Art. 40 is therefore not applicable *at all* in this case, not "marginally applicable", and the name *Tornieriobatinae* must remain the valid name of the taxon, if the latter is to be recognized.

This last example has the merit to clearly tell us what is the philosophy of SAVAGE concerning the validity of family-group names. He in fact favors the rule (R2) above, and considers that, as far as possible, one should suppress family-group names which are based on



generic names currently considered junior synonyms of valid names. He then tries to use Art. 40 for this purpose, and not for the real purpose of this article, which is to *protect the stability of nomenclature* by conserving *well-known* and *widely used* names. It is also clear that it is for this same reason that SAVAGE (1973: 354) used the name *Platymantinae* for the taxon named *Cornuferinae* by NOBLE (1931: 521), after the ICZN had decided, in a very disputable Opinion, to consider the name *Cornufer* as a junior synonym of *Platymantus* (for a more detailed discussion of these points, see DUBOIS, D-11).

The exactly same philosophy is adopted in ASW, and I now understand better, in the light of SAVAGE's (1986) paper, some of the choices made in this list (concerning e.g. the names *Xenopodinae* and *Petropedetinae*), or the strange statement on p. 105 of ASW, which had elicited my surprise (see D-41), that *Alytidae* was the first available name for the family currently known as *Discoglossidae*, while I had clearly shown that the first available name for this taxon was in fact *Bombinatoridae*: it is because, this latter name being based on a junior synonym, it was simply considered by the authors of ASW as virtually non-existent! However, as I discussed in detail elsewhere (D-26, D-37), these matters are not so simple, and such practices cannot be accepted.

The refusal of respecting the principle of priority for family-group names has important consequences. It is almost equivalent to a refusal of accepting that family-group names be ruled by the *International Code of zoological Nomenclature*, and is therefore a step backward, contrary to the tendency which has always been increasing to elaborate and use precise rules for the use of names in systematics. It should therefore not be accepted by taxonomists who wish to work for a stable system of nomenclature of living beings.

Incidentally, this case also throws a light on another type of errors which had struck me in ASW, and which is mentioned elsewhere (D-41), but the reasons for which I had not really understood. Thus for example this list states that the type species of the genus *Lepidactylus* is *Rana fusca* Schneider, 1799, while it is in fact *Rana typhoma* Latreille in SONNINI & LATREILLE, 1801. The reason of this mistake is the same as that just discussed for family-group names: since this latter name is currently considered a subjective junior synonym of the former one (HEYER, 1968; LYNCH, 1971), the authors of ASW replaced the true name of the type species of the genus by the valid name of this species. This clearly shows a confusion between the concepts of nominal species and of biological species: two different nominal species may be considered to be the same biological species, they nevertheless remain distinct nominal species, and the type species of genera are nominal species, not biological species<sup>3</sup>. A symmetrical case is that e.g. of *Necturus*, where the reverse mistake was done: ASW considered FITZINGER's (1843, designation of "*Nectur. lateralis* Wagl") (in fact *Triton lateralis* Say, 1823) as type species of this genus to be valid because this name is currently considered a subjective synonym of *Sirena maculosa* Rafinesque, 1818. However, the nominal species *Triton lateralis* was not part of the originally included species of *Necturus* Rafinesque, 1819, and is not eligible for type fixation in this genus. Therefore, BROWN (1908,

3. This is not understood e.g. by ZHAO 1984, who states that the type species of the genus *Liuia* Zhao & Hu, 1983 should move from *Ranodon wushanensis* Liu, Hu & Yang, 1960 to *Hynobius shiki* LIU, 1950, since the former is a synonym of the latter: both nominal species may be synonyms. RISH & THORN, 1982, the former nevertheless remains the type species of the genus *Liuia*, by original designation. Let us note also that the name *Liuia* is misspelled *Liuia* in ASW pp. 565-566.

is the author of the valid type species designation (see Table VII in D-41). Other mistakes of this type may exist in ASW, where I have not looked especially for them.

#### CLASSIFICATION USED

This last point deserves little comment, except that it is in error that, concerning the few changes that I had proposed (D-19, D-26, D-29) to LAURENT's (1980 a) classification scheme, SAVAGE (1986 : 262) writes : "The bases for these decisions are not discussed" It should be clear that my paper (D-26) was dealing with *nomenclatural* problems, not with classification itself. However, nomenclature is meaningful and can be used only on the basis of a classification, and I had to choose one, even if I did not ignore that it could not be the "definitive" one! I chose LAURENT's (1980 a, 1986 a) scheme and made clearly reference to it. In the few cases where I introduced (slight) changes, in particular in the recognition of subfamilies, I clearly gave *references* to works justifying these choices (Discoglossidae, pp. 24-26 ; Pipidae, p. 27 ; Pelobatidae, pp. 28-29 ; Bufonidae, pp. 33-35). I should stress that in all these cases I proposed these changes as tentative, but only using already existing names, in order not to create new taxa or names. In one case I even used voluntarily an artifice in order not to create a new name. This is the case of the subfamily I suggested to recognize for East Asian bufonids (including the genera *Ansoma*, *Bufoides*, *Leptophryne*, *Pedostibes*, *Pelophryne*, *Pseudobufo*). No family-group name, based on one of these family-group names or on a synonym of one of them, exists at present, and I could certainly have created one. However, since we clearly still lack much information to prove that this group is really homophyletic, I refrained from doing so, and used the following artificial solution. There currently exists a family-group name Adenomidae Cope, 1860, based on the generic name *Adenomus* Cope, 1860. The type species of this nominal genus is *Adenomus badioflavus* Cope, 1860, a name which currently stands in the synonymy of the name *Bufo kelaarti* Gunther, 1859. Finally, INGER (1972 : 360) has suggested that this latter species, from Ceylon, might have to be retired from the genus *Bufo* and placed in a genus of its own. I therefore proposed to *provisionally* use the name Adenominae Cope, 1860 for the subfamily of East Asian bufonids, pending further studies on the status of *Bufo kelaarti* and of the other species of this group. This action has the double advantage of avoiding the premature creation of a family-group name for a poorly known group, but also to draw the attention to the interest of a study of relationships within this group and between this group and other bufonids, and I hope it will prompt studies in this field. This is clearly a more economical and prudent attitude than that of many other authors, not only in the past but also in recent years, like e.g. SAVAGE (1973), who proposed two new family-group names (Allophrynidae, Platymantinae) for taxa which were not even defined or diagnosed, and the validity of which was not at all demonstrated in his work (actually, the second of these taxa is clearly heterogenous and invalid, at least as proposed by SAVAGE ; see D-2, D-11, D-35).

#### CONCLUSION

After this detailed analysis, I regret to say that I must reject as wrong *all* the corrections proposed by SAVAGE (1986) to my list of suprageneric and generic names of anuran

amphibians (D-26), and that I reject his statement that "the materials (especially the generic lists) presented [by Dubois] require considerable modification." This does not mean that this latter work contained no mistakes or omissions, but these were all missed by SAVAGE (1986). Since my work was submitted for publication, and apart from the changes made necessary by the new *Code*, which were "corrected" elsewhere (D-29), I have found 7 errors and 5 omissions in it. Four of the errors concern the first use of subsequent spellings of family-group names: the spelling Centroleninae was first used by BRATTSTROM (1957 : 73), not LUTZ (1969 : 276); the spelling Phrynomeridae was first used by PARKER (1932 : 1241), not PARKER (1934 : 9); the spelling Ranini was first used by BONAPARTE (1839 : [225]), not BRONN (1849 : 684); the spelling Ranidi was first used by BONAPARTE (1837 : [248]), not ACLOQUE (1900 : 489). The fifth error concerns the author of the first family-group name based on the generic name *Rana*, which proves to be GOLDFUSS (1820), not GRAY (1825) (see DUBOIS, D-29). The two remaining errors concern the family-group names Allophrynyidae and Platymantinae, created by SAVAGE (1973): I had not realized that these names were not accompanied by any diagnosis and are therefore nomina nuda in this work; the first one obtained a status in nomenclature in GOIN, GOIN & ZUG's (1978) work (see DUBOIS, D-31), and the second one, which is a junior synonym of Dicroglossidae Anderson, 1871 (see DUBOIS, D-26, D-35), obtained a status in nomenclature, under the spelling Platymantini, in LAURENT (1986 a : 760). As for the omissions, four of them also concern subsequent spellings of family-group names: I had missed the spelling Pelobatidea, due to HUXLEY (1871 : 189), and the spellings Pipini, Bufonini and Hyladini, used by BONAPARTE (1839 : [225]). Finally, I only recently (D-35) rediscovered the family-group name Colodactyli Tschudi, 1845, which should now stand in the synonymy of the name Discoglossidae, and entails a change of date for this name.

The total number of names mentioned in my checklist (D-26) being 572, the total EO rate for this whole work is therefore of 2.1% (E rate : 1.2% ; O rate : 0.9%), which seems acceptable according to the a priori criterion proposed elsewhere (D-41). A few additional mistakes or omissions certainly remain to discover, but this should not significantly alter these figures.

## RÉSUMÉ

L'auteur répond aux critiques diverses récemment émises par SAVAGE (1986) sur ses propres travaux (notamment DUBOIS, 1984), et montre que celles-ci sont dues à de graves incompréhensions de plusieurs importantes règles de nomenclature. Une attention particulière est accordée aux problèmes posés par la distinction entre différents types de noms et d'orthographe (nouveau nom, nouveau nom de remplacement, émendation injustifiée, orthographe subséquente incorrecte), et plusieurs exemples sont traités en détail. Il est montré que certaines des suggestions de SAVAGE (1986) concernant les noms du groupe-famille reviennent en réalité à une proposition de retour à des règles antérieures à celles du *Code international de Nomenclature zoologique* pour ces noms, puisque cet auteur n'accepte pas l'application du principe de priorité à ces noms. La question des règles suggérées par DUBOIS (1984) pour la nomenclature des taxons du groupe-classe est aussi discutée, et les modifications récemment proposées à cet égard par LESLURE, REOUS & GASC (1986) sont critiquées.

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*Note* . — To save space, the references to the works cited above which are given in the papers D-8, D-11, D-25, D-26, D-29 and D-41 are not repeated here (the references D-1 to D-40 are given in D-41).

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